MR. SHETTEL: That should be perhaps part of the historical record, and it would be very interesting.

MR. LEE: Well, I know as the authority responsible for regulating water use, I think that might be an issue that the State of Nevada may have a better sense for. I am somewhat removed from the data.

MR. SHETTEL: That is probably part of the State Engineer's database, perhaps. I don't know personally. Just an idea.

MR. LEE: Thank you.

CHAIRMAN HORNBERGER: Thanks a lot, Mike. We have had enough feedback to lead you to your next three papers. I think we are going to proceed, and I believe everyone is here. We know that our speaker is here.

And so we have a program next that is for a DOE scientific update, and we have several things, or two things, two main things that we are going to consider this afternoon.

The first is an update on the Chlorine 36, and I think probably everybody knows that the finding of Chlorine 36 at the repository in Horizon at least five years ago led to some reappraisal of fast flow paths, and potential fast flow paths to the repository arising.

And later there was some -- a different laboratory had done some analyses and there is now some work trying to work towards a resolution of differences that were observed.

So, Zell Peterman is going to give us an update.

DR. PETERMAN: Let me mention before I start that there is a significant part of the Chlorine 36 validation team here today. Bob Robeck from Los Alamos has taken over the work down there, and Greg Nimz from Livermore, who actually does the Chlorine 36 analyses, and my colleague from Denver, Leonid Neymark, who had been heavily involved in the design and the sensitize design and the sensitize related to the validation project.

The first slide, I gave something similar to this several weeks ago to the BSE Project Oversight Board, and Bob Thorsen (phonetic) observed that I had 15 pages of history and no conclusions regarding the validation project, and nothing has really changed.

But let me just jump to the conclusions first, and then work our way through this history. We thought it was important to try to give a historical perspective as we think we understand it.

And over the last 3 years, we have generated a lot of data, and we have given a lot of thought on how to try to validate the work. We have done a number of experiments, and we have a lot of information.

And our immediate goal is to sensitize and integrate all these datasets in to a report that is due in December. And that in that report that after doing all of this, and really having time to think about the data, we will develop a path forward. Right now we don't have that. The report is our path forward.

But there will be in that report presumably a path forward that leads to hopefully some sort of resolution. And that is kind of where we are, and let me just go through this.

I have a lot of slides, and I don't want to go and read every bullet, but let me just try to summarize. Sometime in early Fiscal Year '96, when the ESF was being constructed, there were two studies that were started.

One was Chlorine 36, and the other was a study of fracture minerals, fracture minerals being the only physical evidence of percolation through the unsaturated zone at Yucca Mountain.

Los Alamos conducted the Chlorine 36 work, and USGS conducted the fracture mineral study, and basically we both sort of followed the TBM as it made a tunnel and collected our respective samples. Next slide, please.

Early on when it was evident when elevated Chlorine 36 values were found, we had a meeting in Denver, and the Los Alamos' folks, and the Denver folks, and we really struggled with what this meant, and how we were going to validate it.

We talked about doing deturium, technesium 99, and iodine 129. There was a very early attempt by the USGS to look for tritium and that pretty much failed because samples were collected from the tunnel walls, and those tunnel walls had been saturated with construction water. Next slide.

The Chlorine 36 worked and continued to the ESF, and into the ACRB as it is referred to. Technesium didn't really get off the ground, and it is really a tuff thing to do.

The work on the fracture minerals, we developed a spectrum, a dataset, for the uranium series that ranged from a few thousand years, a few tens-of-thousands of years for the youngest, outer-most materials, to well over a half-a-million years for the older material.

And then this evolved into a uranium lead-dating system, which now pushes the formation of the older parts of the fracture minerals back to 10 or 11 million years, within a million years or so of when the tuffs were formed. Next slide.

In 199, and I think it actually started in late '98, the DOE asked the USGS to organize a validation project that could independently verify the presence of bomb pulse Chlorine 36 or not in the exploratory studies facility.

The final proposal, and what we put together, involved the USGS, Lawrence, Livermore, and AECL, and Los Alamos, as an oversight -- to provide oversight for the validation work.

The first organizational meeting was held in the spring of 1999. Next slide. This was the dataset at that time that we were asked to look at and basically on the wire access to the Chlorine 36 over chloride ratio, times 10 to the minus 15, and it says maximum twice the same.

And that was considered anything above that line was considered to be bomb pulse. The little XXs is just distance from the north portal through the

ESF, and the anomaly in the middle there is associated with the Sundance Fault.

And we refer to that as the Sundance Anomaly. And just another point, there is an anomaly to the left of that that is composed of about five samples, and that is the drill hole life feature. So that is kind of what we were looking at. The next slide, please.

So we tried to design a sampling from it, and we decided to look at the Sundance anomaly, and the Drill Hole Wash anomaly. And we went to the tunnel, and we looked at all of the sample sites, sites that had been sampled by Los Alamos.

And we looked at all three maps to assess fracture spacing and that sort of thing from the Bureau of Reclamation mapping. Because of the -- the dataset that you just saw was developed from samples that were largely collected from the right rib of the ESF, the lower quarter, because a lot of them were collected by jackhammer.

And by the time that the validation work started, that lower quarter of the ESF had been washed down so many times to clean walls or control dust, that it was decided that it decided that we were not going to try to collect samples from there again. So the next slide, please.

So it was decided that we would build four meter long bore holes, dry drill 50 of these, and 40 spaced along the Sundance anomaly and 10 spaced along the drill hole wash anomaly.

It had several advances. It goes us in past dry out and it got us in past infiltration by construction water. A lot of the surface or tunnel wall samples had to be corrected.

The data had to be corrected for the presence of construction water, and by going in four meters and preserving the core, then we could also extract water and conduct treading analyses.

One thing I have failed to include in this history is that there was a peer review panel at the Chlorine 36 dataset, and that peer review, one theme that kept recurring is that you have got to go in and try to do tritium.

So this was an opportunity also to do tritium. Next slide, please. We were delayed at that point, and there was a multi-month safety stand down. I can't even remember what caused it now, but that delayed things for several months.

There was a bit of a problem in getting all the perceived QA procedures going at Livermore.

Anyway, the holes were finally drilled, and we looked only at the deeper two meters from the construction water and dry out.

We sub-sampled or we sent samples for Lawrence Liverman, and we took samples to Denver for water analysis and tritium, and we sent samples to AECL for uranium isotopes.

The Livermore -- the first Livermore dataset was developed by an active leaching process, with seven hours in a rotating tumbler; in contrast to the previous Los Alamos methods, which was a passive leach for 24 to 48 hours. Next slide.

The first Livermore results were presented at the NWTRB Chair meeting in Pahrump, and the values were lower than had been observed, and basically it was concluded that that leaching technique was probably too aggressive, and we were getting too large a component of rock fluoride.

If the rocks are multiple reservoir chloride, there would be chloride initially in the volcanic rocks, and I think the average for the high silica is something like 170 ppm chloride, and this is primary chloride.

There would be chloride in the four moderate in there would be chloride in fracture order, and presumably what you want to look at for finding bomb pulse is to try to look at fracture water, which you can't -- nobody has sampled fracture water, but you can sample the salts. You can leach the salts.

So you try to balance the leaching to maximize the meteor component, and minimize the rock component. Anyway, next slide. So there was general agreement that the dynamic leaching was a little too aggressive, and there was an agreement among all participants at that time that we needed really to rest, have a sample to test the bleaching process.

And the USGS was charged with preparing that sample, which we did. TRB too a very intense interest in this, and wrote a letter to the OCRWM Director urging a quick resolution, and that was on June 16th of 2000. Unfortunately, we are still not there.

We developed a path forward, and we got a large sample from Niche-5 in the cross-drift. This was crushed and sized in Denver, and aliquots were sent to both Livermore and Los Alamos to conduct leaching studies. Next, please.

These results were discussed at several meetings, and there was a meeting in November of 2000 at the GSA meeting in Reno. Next slide. The bottom line is that it was decided that the best way to go about it was a passive leach, and to minimize the time.

And at that time one hour was sort of indicated as a desirable time for leaching of that size of a fraction rock, even though that was in somewhat of a contradiction with the earlier dataset, where samples were leached from 24 to 48 hours. Next slide, please.

So we needed to go back now and look at the validation core again, and the approach this time was we would crush the samples, and actually the sample management facility crushed the samples, and some of the remaining core, and this was done in basically a brand new crusher.

The only thing that it had ever seen before was other samples of the Topopah Spring type. Samples were transported to Denver, and the USGS leached the samples, and distributed aliquots of the leach samples to Los Alamos and Lawrence Livermore, both of which then spiked the samples with different chloride isotopes, and prepared the silver fluoride precipitates, and Lawrence Livermore ran the samples.

And generally the results were in fairly good agreement between samples prepared at Los Alamos and samples prepared at Livermore. The numbers ranged from 200 times 10 to the minus 15, to 500 times 10 to the minus 15, still lower than the previous Los Alamos dataset.

At a meeting last January, we convened the group in Denver, and we looked at the data, and there was one dataset in the old Los Alamos data where core from Niche-5 had been analyzed, or I'm sorry, Niche-1, had been analyzed, and something like 8 out of the 10 samples that were analyzed revealed an elevated chloride 16 value.

And so we thought, well, this is what we need to do. First of all, we had a hard time finding the core. It turned out that some of it was in the USGS hydrological research facility, and most of that had been used for physical property measurements, or had been saturated with J-13 water, and so on and so forth. But there was still a pretty good collection at Los Alamos. So we split the core up. Next slide, please.

And we agreed that we would do -- there was concern that machine crushing might yield too much fresh rock fractures, and therefore, overwhelm the leachable chloride with rock chloride.

So we followed a procedure used at Los

Alamos, which was hand crushing on a steel plate and a hammer. Los Alamos conducted their or analyzed their six samples, and they reported ratios of 1140 times 10 to the minus 15, to 8580 times 10 to the minus 15.

That is the highest or largest number that has been reported so far. Chloride concentrations were 1.3 to .67 milligrams per liter, and we processed what should have been roughly an equivalent core in Denver, and we got ratios between 244 and 708 times 10 to the minus 15.

Both groups had monitored leaching blanks during that time and no leaching blanks were deemed to be acceptable. So that is the most recent puzzlement as to why these numbers differ.

CHAIRMAN HORNBERGER: Can I ask a question on this, Zell? On the previous go around, the USGS did the leaching and distributed the aliquots. Here two different labs did the leaching.

Why did you do that apart from -- am I reading this slide correctly, that leaching was done both by USGS and Los Alamos? Whereas, previously it was done just by USGS?

DR. PETTERMAN: That's correct, and it was because of that, because previously it had shown that if one lab leached a sample, and distributed the liquid leaches, both labs could get the same answer.

So we were back to -- and we had already demonstrated that to be true. So now we had another chance, and the early Los Alamos data had said there were elevated values, and so we just decided it was best to let's just let those -- we didn't physically split it. It was pretty rumblized, and so Bob Robeck had inventoried what was available.

And we took alternate -- I don't know, either one foot or six inch segments of rumblized core, half to Denver and half to Los Alamos.

It should be, you know, unless fate is really cruel, they should be comparable. The statistics, the probability, of them being or leading to these results is extremely low.

The bottom line though was that we got different results, and again the leaching blanks were okay at both laboratories. So we decided that one thing that we did not have control on was the actual crushing blanks.

So we got a hold of some computer chip silicone from the DOE lab in Golden, the Energy lab, and supposedly pure to six figures. And we crushed it just like it were a rock, and using the same equipment, and we also conducted a systems blank at that time.

Unfortunately, our system blank, which basically is pretending we have a rock and leaching it, but there is no rock in the pan, our system blank was a bit higher than what we had seen before, which has confounded the issue.

But if we correct our crushing blanks for that leaching blank, then our blanks, the crushing blank, we have concluded is not a significant issue by the USGS in Denver.

At the same time, Bob Robeck had surplus material from one of the core samples, which he sent to Denver, and we leached it, and we got essentially the same number that he did, 1130 times 10 to the minus 15.

So that we could confirm, and that is kind of where we are at the moment. And I think that it is very important, and that we have so much data now, and so many efforts to try to resolve this issue, that let me try to go through the conclusions here.

So this is kind of a summary. The old or the early dataset at Los Alamos, samples from both ESF and Niche-1, and this is the Sundance anomaly now, had elevated Chlorine 36 values.

The Los Alamos data on the Niche-1 core, the most recent analyses, had elevated Chlorine 36 values. An early effort, and I think six samples of the original Chlorine 36 validation core were analyzed at Los Alamos from the Sundance.

Those did not have elevated values, but the numbers were in the normal background range to the Los Alamos dataset. Next slide.

The lowest values measured was that original dataset at Los Alamos, or I'm sorry, at Livermore, and the active leaching. And then next we found no bomb pulse in the validation core holes, and we found no bomb pulse in the Niche-1 samples. Next slide.

So I think we are at a critical juncture here, and it is extremely important that we have the time to sensitize and integrate the existing data, and after doing that, then come up with a path forward.

And to be honest, we just don't know what that is at the moment, but we think that putting all these data and having time to think about the data in a report is a next very logical step.

The project has indicated that they could bring one or more outside experts in to review the report and whatever path forward we come up with.

Let's go to one of the illustrations here, and maybe that summarizes -- let's see, how about page 29.

These are all the data now plotted on the -- the Y axis is one over chloride, and the reason we do that is that in this ratio concentration space, if you plot the reciprocal concentration, then binnery mixing comes out as a straight line. That is the only reason.

But the chloride concentrations is also shown on the upper access. The triangles down in the lower left-hand part are the Livermore results, and the active leaching of the chloride validation core.

So that is one set of data. The solid blue diamonds are the original Los Alamos dataset for the Sundance Anomaly, and this is all Sundance Anomaly. The orange triangles are the results, the second round of results on the Chlorine 36 validation core processed and leached in Denver, and analyzed at Livermore, but aliquots also to Los Alamos, and spiked at Los Alamos, and analyzed at Livermore.

And those are the interspersed green triangles in that field of orange triangles. So there is general agreement, and then the largest value is that kind of open diamond, and represents the most recent Los Alamos data on the Niche-1 core.

And the little purplish triangles down amongst the orange ones are the USGS results on the Niche-1 core, both analyzed by Livermore. So again that is kind of where we are, and I know that it is not satisfying, and I think we have made progress.

I think we need three months now to prepare the report, and I think we have to go into what I would call kind of a forensic mode, and we have got to really get into the old dataset, and really look at it hard, and see if there is anything in there that would be of interest in reconstructing how this has evolved.

CHAIRMAN HORNBERGER: All right. Thank you. Questions? Raymond.

VICE CHAIRMAN WYMER: It must be a little disappointing to you that after all this time that we still have something unresolved.

DR. PETERMAN: It is extremely frustrating.

VICE CHAIRMAN WYMER: But there is a suggestion at least that at least to the Sundance Fault, that there is some evidence for fairly rapid movement of water into the repository horizon, and that is one part of the two-part equation, and how fast does it move.

But the second part is what volume moves, because not much has moved, and you don't really care with respect to the proposed repository. What do you know, or what do you plan to know, or what does somebody plan to know about the volume?

DR. PETERMAN: Well, I think that is more of a modeling exercise and Los Alamos has addressed that, and has concluded that the actual volume of water is probably small.

Now, I see that there is a flaw in this presentation. The dataset that I didn't mention was the tritium data, which we have also done on these 50 core. And there again we have got another disconnect.

And in the Sundance Anomaly, we find no tritium of any consequence. I mean, no tritium, period. It is down to one tritium unit. In the south ramp, where there is no elevated Chlorine 36 values, we find significant tritium values.

So we have an anti-correlation between tritium and Chlorine 36, even though the peer review said that tritium is the ultimate hope for validating the Chlorine 36.

But you can come up with post-hoc explanations for tritium, and it is going to move into the vapor phase, and Chlorine 36, probably not. So you can come up with reasons why they might not agree

DR. GARRICK: How much cross-checking of samples has there been in the analysis? Different labs and even outside of the established --

DR. PETERMAN: I would refer that question to Greg Nimz, who actually conducts the analysis. I'm sorry, but I a not sure that I understand the question. Are you asking how much cross-checking within the samples that we have done in the last two years under this validation, or cross-checking in general between laboratories?

DR. GARRICK: Let's try and answer both of them. Both sound interesting to me.

MR. NIMZ: Well, the best cross-checking is probably the samples that were prepared at Livermore Laboratory and at Los Alamos, and a little more at the Livermore Laboratory, and we get very good agreement as Zell pointed out in those.

Cross-checking around the world has not been done except for sample response activity, where one lb send this to the -- the same sample or a similar sample, to two different laboratories for purposes of turnaround time and that sort of thing.

And then in general analyses, the clean

laboratories, especially I am familiar with the prime laboratory in Indiana and Livermore. Those analyses have generally compared very well.

DR. GARRICK: Have the results had any impact on the models that are being used to analyze radionuclide transport?

MR. NIMZ: I don't know the answer to that.

DR. PETERMAN: Let me ask this question of Abe Van Link, and of course, and he says no.

MR. VAN LINK: since we assume that this data is correct, and it is fully incorporated into the modeling, and until some definitive group comes in and says that it isn't correct, we would not change the model.

However, the very fact that we also have some tritium in the south ramp shows that some very small fraction as the model now indicates can move rapidly. So probably the model wouldn't change anyway even if this data came in. But it is a scientific credibility issue for us.

DR. GARRICK: Thank you. Has there been any indication of any gradance of this transport of the chlorine, any particular location that has indicated a more definitive flow pattern than maybe you knew about before?

DR. PETERMAN: Well, the original dataset has been used or explanations have been put forth on that slide number six, which is the original Los Alamos dataset.

Again, there are contradictions. The south ramp, among the whole of the ESF, the south ramp is the most broken up piece of rock. It is highly pallid, and there are fractures there that when it was drilled, it was breathing to the atmosphere and blowing to the atmosphere.

And the contradiction there is that there have been no bomb-pulse Chlorine 36 values found there, but again there is tritium there, and so it is still a set of contradictions.

And with those sorts of contradictions, I guess I would be personally reluctant to say that I am going to use these patterns to say too much about specific flow paths or flow zones within ESF, because there is still something that we don't understand.

CHAIRMAN HORNBERGER: Zell, let me try to summarize what I take from your presentation. The accelerator mass spectrometer appear to work. That is, they give you the same answer if you give them different aliquots.

DR. PETERMAN: That's right.

CHAIRMAN HORNBERGER: You get, however, different answers when different labs prepare or do the crushing. So am I right in inferring that this would either indicate that the USGS crushing adds an anomalous amount of dead chlorine, or Los Alamos adds an unusual amount of elevated Chlorine 36; is that a fair assessment?

DR. PETERMAN: I think that is a fair assessment. That's one thing that we tried to look at by this crushing blank, which turned out to be somewhat confounded by the fact that apparently a leach wire suddenly was higher in chlorine than we thought it was when we actually did the earlier samples, or it was higher than when we did the earlier samples.

So we have to make some assumptions about calculating the crushing blank. If we use the leach blank that was conducted at the same time as the crushing blank, then we conclude that crushing doesn't add anything significant.

But it is a complication that makes one feel a bit uncomfortable still.

CHAIRMAN HORNBERGER: And I take your point that you really need three months to reflect on this and come forward with a plan, but in general terms, do you anticipate that it might be reasonable to plan to involve other groups, groups that have not yet been involved in the process, in terms of trying to resolve this?

DR. PETERMAN: I think the project is considering that. I don't know if the DOE wants to make a comment on that.

CHAIRMAN HORNBERGER: My question wasn't what the project was considering. My question to you as a geochemist is would that make sense?

DR. PETERMAN: Yes, I would welcome that, personally welcome that, you know. Anything to get this off of dead center.

CHAIRMAN HORNBERGER: Milt.

MR. LEVENSON: I have got a couple of questions. In one of your backup slides, you identify that the mechanical crushing equipment at Los Alamos was found to be contained with chlorine 36.

Now, that contamination didn't originate in the crusher. What are the chances of other things in that laboratory are also contaminated? Has there been a sort of forensic search of that laboratory to make sure that it is a clean laboratory?

DR. PETERMAN: Bob Robeck, who has taken

over the Chlorine 36, actually works in a different laboratory than that earlier work was conducted in. The contaminated equipment was reported in that earliest Chlorine 36 report.

It was detected and that's why basically they went to the steel plate and hammer rather than the mechanical crushing.

MR. LEVENSON: But contamination at the level of 10 to the minus 15, some of my experience is that something in a building is contaminated, and everything in that building might well be contaminated at that level.

And changing equipment, or even the lab next door, doesn't necessarily help. The other question that I had in connection with the anomalous tritium, I have the impression, and like many impressions, it could be wrong.

But I have the impression that some of the drilling equipment that the DOE is using or has used is recycled equipment from the testing station. Has anybody looked seriously as to whether the tritium is contamination brought in my drilling equipment?

DR. PETERMAN: Early on -- and this is only sort of an antidotal recollection on my part, but there was some contaminate drilling equipment used in some of the surface-based drilling.

The drilling that was done underground, we used brand new core barrels, and brand new bits, and new core liners, in anticipation that we did not want to have that possibility.

And the possibility that through the ESF, through the Sundance, and drill hole wash anomaly, we don't find any. And the same equipment was used in the south ramp, and we sort of would say that equipment is not a problem.

There was also in the lab, the survey lab, there were early problems. The exit signs were triturated, and so that created problems. And your watch, if you have a triturated dial, you don't want to be in there when you are extracting water. So, yes, it is a tuff ball game.

MR. LEVENSON: Is the tritium contamination in the south ramp been found in cores or only in surface material?

DR. PETERMAN: The south ramp is water extracted from dry bill core. Those are all by vacuum distillation, and taking the preserved core, and distilling it in a vacuum line.

CHAIRMAN HORNBERGER: Staff. Andy.

DR. CAMPBELL: Thanks. I have a lot of

questions. Andy Campbell, ACNW staff. But I am going to try to touch only a couple of them. Why is Iodine 129 not done? Is there a technical reason?

And the reason that I ask that is Chlorine 36 was produced in the '50s by bomb testing in the Pacific, because of the irradiation and activation of chlorine in the sea salt.

Tritium was actually mainly produced in the tests in the atmosphere, in the hydrogen bomb tests in the '60s after the breakdown of the test data. The iodine, on the other hand, also has a source from pre-processing in Sullyfield and the other reprocessing plant in France.

And, of course, various programs around the world have been putting out Iodine 29 for a long period of time. So if you are seeing the penetration of these isotopes to the repository, then Iodine 29 might be a good trace, that of more recent activity, as compared to activity produced in the '50s and early '60s.

That is a question I guess for you, and then I will ask another.

DR. PETERMAN: That's interesting, as we were just talking about that at lunchtime. When Mark Haffey was doing the work at Livermore, he was moving in that direction, and I don't really know how far he really got. Drake would know. He took a position at Purdue to oversee the AMS facility there.

And so basically we have not pursued. Greg, do you want to say anything about 129?

MR. NIMZ: Yes, the only point I would make is that it would be analytically very difficult, really tuff right now with the amount of chloride that we are getting from these samples.

And the amount of iodine is going to be much less. So there is a very big question as to whether we would even be able to analyze the iodine, which would occur in concentrations of perhaps of a factor of a hundred less than chloride.

So there is that analytical junk that we would have to make, which would take several months of preparation to just understand whether we could do iodine with these very little concentrations when we are doing this passive leachings.

DR. CAMPBELL: Okay. The next question or questions has to do with the approaches used to resolve contamination when you are doing trace analyses.

This certainly is the first example of a contamination issue, and the fact that virtually every trace analysis of either an isotope or of a metal have involved a number of years of kind of floundering around until everybody agrees on a methodology, and everybody agrees on an approach, and the way to do it, and then people start getting consistent results.

Part of that process involves systematically going through and identifying every single possible source of contamination in every step along the way. And it is not clear to me at least from how these analyses have been done in terms of the selection of samples, and not really analyzing the same thing.

And it is not clear, for example, that a reference material has been produced that has a known concentration that each lab can include in a set of samples to check on the validity of their analyses.

You typically do a check sample that is very similar in matrix to the samples that you are analyzing. Part of the problem, for example, is doing distilled water and leech blanks, is that you don't always get the same activities going on that you would if you include a crushed sample and so on.

And there are all kinds of wrinkles on this process, and it is very detailed, and it is very obsessive for the analyst to do it, but it has to be done to eventually ferret out if there is in fact a contamination issue.

Is that all going to be what you guys have done folded into this report so that an objective outsider can say, ahha, have they looked at this area and have they looked at that area.

And are there any further activities that you plan to do to try and nail this down. The other thing that people have done are inter-calibration exercises, where they take the same sample, and distribute it to half-a-dozen or a dozen labs to do that analysis.

And let each lab work up that sample, and then do a comparison, a blind comparison of the results, to see if any particular lab either has either or very low numbers, and could you comment on that?

DR. PETERMAN: Well, I guess I would agree with everything that you said there. It needs to be done, and we have probably done some of it. I think we will address those issues in the report, and it will be part of our recommendations for a path forward.

Part of it, you know, is always a resource issue. You know, it is expensive analyses, and a collection of samples that are less labor intensive, and it doesn't take very long to burn up your budget.

And that is always an issue, but I agree with everything that you said. I am certainly aware of some of those historical problems in working at that level.

We asked Greg at lunchtime how many folks the world over have rocks that have chlorine 36, and he said it is only you guys. I think there was an additional comment there which I won't pass on.

So the point is that it is not something that is routine, and we do need to think about everything that you said.

DR. CAMPBELL: One last comment on the view graph up there at the three different years worth of data. The interpretation as I recall from the '97 report was that the high spikes that are categorized as bomb-pulse above the maximum level were interpreted to be bomb-pulse in association with fractures or faults. There are a few exceptions, but mainly those data are.

But below the maximum and above the lines, there is a lot of scatter in the data until you get to 6,000. And then the data gets very tight. And there were two explanations for that that I am aware of.

One was that something happens at 6,000 that causes a flushing of the system, and the scattering of the data before 6,000 might be representing different amounts of pre-plisticing (phonetic) water of different Chlorine 36 contents due to changes in the magnetic field.

Bill Murphy at the Center did a statistical analysis, and said, well, you could explain all of that scatter below 6,000 as simply a two-hand mixture of bomb-pulse contamination and modern water pre-bombed modern water.

If that is the case, then it seems that you actually have to nail this issue down even if the model attempts to take into account fast paths, because the one interpretation might be that that scatter represents a lot more fast paths than just a few fractions.

You could certainly reasonably interpret that data in that way. This is real and not due to contaminated samples, and then that would suggest that its more important than just for a few fractures. It might be important for a significant portion of the rock.

DR. PETERMAN: Well, that's true, and also that a similar lab arrived at a similar interpretation, and that could explain all of that. Most of it, except for the south ramp, and virtually every sample, or most every sample there has a little bit, variable proportions of bomb-pulse chlorine 36, and reasonable interpretation.

MR. ROBECK: I am Bob Robeck from Los Alamos, and I took over the project from June about two years ago, and have been working and puzzled by this issue ever since. It has been a frustrating experience scientifically for me.

There has been a lot of talk -- well, first of all, what you were saying over there, I agree. Where the project is now, I think we have eliminated a lot of first quarter issues that we have been able to come up with through a considerable amount of discussion and meetings.

And we said, well, let's get a reference sample and try to develop a reference sample that we can both work on. We tried that and we tried -- the GS tried leaching and distributing (inaudible), and I cross-sampled and sent them to Zell, and Zell has cross-sampled.

And we are working through the first order problems, and now we still don't have the answers, and now we need to get to the difficult issues to address.

And can we be missing something at the very low level, or perhaps are we looking at more than one problem rearing its ugly head, and from time to time another problem perhaps rears its head at another time.

Personally, I think that's where we are right now, and I don't think we have a single issue, and a lot has been said about the blank issue, and I just wanted to address that.

When I took over the operation, it was shortly after the fire at Los Alamos, and as a result of the fire, I was no longer able to do the work the laboratory that had been used previously by June. So I relocated the entire operation about a mile way in a completely different technical area, and a completely different building.

I vigorously blanked that area, and the blanks came up low, and that area is a non-rad area within Los Alamos. I also modified the procedures so that we could keep careful tabs of the blanks.

Through the course of the analyses now, I have run some 100 samples and no fewer than about 15 percent of those are blanks. And every one of them has come up quite low.

So that any contribution to Chlorine 36 by the blank would not be significant, or would not change any of the conclusions. The blanks that I have taken do not include a crushing blank which is yet an issue.

However, when we do a leach blank, we allow that leach to sit out that length of the time that we take to drive down our samples, which is sometimes up to a week.

Whereas, we are crushing for approximately an hour to maybe a few hours within that laboratory. So I think any kind of fallout that we might get from our crushing equipment, and I don't see where else it could come from because the equipment is vigorously cleaned.

So I think that we have done our best at least to address the blank issue at this point, and perhaps we need to take it a little further. But I also wanted to say that the data that we have generated do not in any way suggest that a random blank is the problem here.

We are not seeing a random high ratio. Rather, we are seeing ratios where they have been determined in the past. So, for instance, he has ditched one sample, and let me jump back.

Of the close to a hundred samples that I have analyzed thus far, only one sample from the cross-drip has what we would consider a bomb-pulse value, which is just barely bomb-pulse value, between 1200 and 1300.

And then when we did this Niche-1 samples, again processing them in the same way, most of them did turn up to have bomb-pulse in the same area where June located bomb-pulse, using modified methods in different laboratories.

Likewise, I processed this Niche-1 samples and did a couple of different experiments, and separated them by size fractions, and you see systematic differences within those size fractions.

And in this case the highest bomb-pulse turned up in the finest fractions, but again the systematics that we see from low ratios to high ratios, and low chloride to high chloride for corresponding samples do not smell like a blank problem.

You would not expect those kinds of systematics. I might also point out, too, June's dataset, where most of these bomb-pulse values that she did find are from her feeder base samples.

Whereas, within her systematic sample set, I believe that only one sample has turned up bomb-pulse. We are certainly concerned about the blank issue, and I am doing what I can to address it further, and we will continue to do that.

But right now I firmly believe that the data does not suggest that a blank is an issue. I don't know what the problem is and hopefully -- and I think that our path forward, we really do need to step back here and look at all of this data. For the last two years, we have been working hard to generate a lot of data, and I don't think we have given the dataset justice at this point. So that is our goal for the next two months here.

DR. RYAN: I am looking at the figure on page 6 and I have been thinking here quietly about statistics. And as the ratio gets bigger, that means that there is more Chlorine 36, right? Yet the uncertainty gets bigger as well.

I would think it would be just the other way around in bars that are shown on this graph, and I don't have the data, and so obviously I am shooting in the dark here.

But as the amount of Chlorine 36 gets smaller, and smaller, I would think the uncertainty and your knowledge of its value gets bigger. I mean, that is just simple sampling statistics to my way of thinking.

But yet it is just the opposite on this graph. So I am stuck with the basic statistics question, and that is when you measure Chlorine 36 and say it is this value, I am stuck with how well you know that. So I am trying to figure out if I should interpret things that are below these various horizontal lines as being different or not different.

And I am kind of stuck with the statistics that you used. I know that this is not a radiometric measure. So it is a different kind of uncertainly analysis perhaps. But I don't really have a feel for how accurate any given measure is.

And I know that you can't do it because you would run out of sample, but if I measured the same sample 50 times, what would the average be and what would the standard deviation be?

What I am reaching for is concepts that we use in radiometric analysis of minimum detectable activity, critical level, and things like that which we can do hypothesis testing.

I mean, you have not talked about that here, and I don't know if you have done that, and I apologize if you haven't. I have not seen it yet. But that kind of thinking may be helpful perhaps. I don't know.

DR. PETERMAN: Yes, it is helpful. Attempts to replicate analyses on individual samples, and June reports this in her reports, has not worked very well. Both data are available.

And so - well, Leonid, do you want to comment on these uncertainties? This is Leonid Neymark.

MR. NEYWARK: Just that we started with the largest uncertainties, for example, for Chlorine 36 and there is a reason for that. But in most cases, and in June's data, a bomb-pulse signal was obtained for a sample with lower total chloride concentration, and it increases the total there in that one.

DR. RYAN: That doesn't help me very much though. The more chlorine 36 you have in the sample, you would think that if the measurement quality increases with chlorine 36 concentration that's not true?

MR. NEYWARK: No, it is not. A higher chlorine 36 total chloride ratio doesn't mean that you have more chloride 36 in your sample. It depends on the total chloride concentration. So if those low chloride samples, you have a higher ratio larger.

DR. RYAN: I guess I would like to follow up if I could. That may not be a meaningful error to report then, because are you measuring the ratio or are you measuring the chlorine 36?

MR. NEYWARK: I think it is that if you have less chloride -- generally speaking, if you have less sample to analyze, your accounting statistics is -- you know, you get less counts and therefore your error is larger, regardless of the ratio of chlorine 36 to total fluoride.

The total amount of chlorine 36 are lower because you have lower chloride sample. Is that true?

MR. TYNAN: Let me first say that I know very little about the data on page 6, because this was not done by me. It was done by the laboratories, and so I am not sure what the meaning of the error is on here. But to answer your question, in general, and to follow up on what Leonid was saying, is that this is simply an accounting statistic problem.

If you have a hundred counts of Chlorine 36, you have 10 percent data. And so if you have or if you are running samples, and if the laboratory chooses to run the samples for five minutes, the samples with more Chlorine 36 will have more counts, and therefore, better accounting statistics.

DR. RYAN: I guess I am getting in a very fundamental question of the accuracy and precision of the measurement relative to minimum detectable levels.

And without some understanding of minimum detectable levels relative to measured levels, it is very difficult to either ascribe or take away meaning from the results.

And I assume that just based on what you talked about that we are at very, very low levels to begin with, and I am just going to try to assess some statistical significance to that, and I have not seen information that helps me to do that yet.

MR. TYNAN: Again, I don't know about the data on this sheet.

DR. RYAN: I appreciate that.

DR. GARRICK: One of the questions that this committee often asks is so what with respect to bottom line health and safety issues. I suspect that you have done enough work now on these ratios on chlorine to be able to categorize what the outcome is probably going to be, in terms of it being one or two, or three different scenarios.

In other words, you probably have a pretty good handle on what is going to be the outcome of your path forward if you had the option of identifying two or three possible outcomes.

Given that, and this is probably a question for DOE, and not to you, but what is the implication? Has somebody considered what the implication might be to the project and to the analysis?

Abe Van Link has already said that the assumptions have sort of embodied in reference to what we were talking about earlier, the possible inability to get any advantage from these measurements.

But I am curious as to whether or not this is really going to have much meaning in terms of the project and in terms of the performance assessment. Abe, this is probably a question for you.

MR. VAN LINK: Abe Van Link, DOE. As I have already mentioned, we fully incorporate the information from the Los Alamos work into our performance assessment at this point.

I think where this comes down now is we need to push to a resolution, because we have several august organizations that we rely on for scientific information, who have come to a point where their own scientific credibility is on the line.

So we need to push forward to a resolution because from my perspective it is in our best interests that we get to the bottom of this, and are able to establish or reestablish credibility for these institutions.

Now, if some contamination is found somewhere, so be it. If they find a new mechanism that one organization was not aware of, so be it. Those are the two or three scenarios that we can come up with.

But either way a resolution will bring us reestablished credibility. It is not something that we want to shove under the rug and say, well, it doesn't matter to performance anyway. We want to get to the bottom of it.

DR. GARRICK: What about if it comes up that there is no bomb-pulse or no evidence of it? Is that going to change anything?

MR. VAN LINK: I hate to speculate on that, because as I said, we do have the tritium work on the south ramp that shows that there are fast paths other than the Chlorine 36 paths, and we do have one tritium sample, I believe, that is associated with a fault in Alco 6 or 7.

DR. PETERMAN: Yes.

MR. VAN LINK: So on the other hand, it probably would change our qualitative understanding of the unsaturated zone. You know, we do have -- most of the water there is pristine water still, and we do have very good evidence from Zell's work that if you look at the bulk of the rock, it doesn't see water very often.

It sees it maybe during an ice age, and so this is still consistent with our current model though, that we have very little water moving through fast paths, and the bulk of the water is resident in the rock for extremely long times.

I think that Mark Tynan was going to say something.

MR. TYNAN: Yes, Mark Tynan, DOE. You covered one of the points already, but the second point that I would make is that if our path forward isn't defined until January, let's say, or the reports aren't out, our ability to resolve this prior to the license application is not a high percentage of success.

So it is likely that this is the ongoing work and post-LA submittal in December of '04.

DR. GARRICK: Thank you.

MR. COBEST: Tim Cobest, ACNW staff. I assume that this is all being done under DOE's quality assurance program, have you had an audit done or anything?

Have you had them come in and give you an independent look at it and come up with anything as far as procedures, and as far as how you clean your test equipment that you were talking about?

You know, handling samples, and have they come up with anything or have they looked at it?

MR. TYNAN: Livermore just had an audit, and -

MR. COBEST: And did they look at this issue?

MR. TYNAN: Yes, and we have I think audits at least once a year.

MR. ROBECK: We certainly have audits of our scientific notebooks and our procedures, and those are ongoing. As far as having and testing equipment, it comes and is examine, but as far as someone actually coming in and observing a procedure that doesn't happen.

MR. LEVENSON: The conversation has been focused on Sundance, but in the original samples, and in fact the highest Chlorine 36 ratio was not at Sundance, was a 2,000 meter and five separate samples indicating bomb-debris. Is 2,000 meters still part of the Sundance?

DR. PETERMAN: It is part of the drill

hole life structure, and that was in our initial plan. We allocated 40 of the bore hills to the Sundance and 10 to the drill hole wash.

MR. LEVENSON: And I gather that there have been some more recent samples that confirm the early Sundances, and has there been any recent samples concerning the early high ones of 2,000 meters, especially since the very highest ones were there?

DR. PETERMAN: Not that I am aware, no, according to the reports. The report data, that is the original data, or the early data.

CHAIRMAN HORNBERGER: I just wanted to make sure that we are clear on this now. Milt said that from your 40 samples that you have confirmed high chlorine-36 ratios at the Sundance? That wasn't my understanding.

DR. PETERMAN: No, we haven't. Not in the validation core, we have not.

CHAIRMAN HORNBERGER: I just wanted to make sure that we are clear on that. That the disagreement was the Niche-1 samples; is that right?

DR. CAMPBELL: One last comment here is Mike Ryan's observation of the statistics. Has anybody done an analysis of the statistics of these high chlorine-36, but low chloride samples that are heavily in the bomb-pulse area?

And that is a very curious result to me, and is there an explanation for that? If you look at everything about the 1250 line, most of those samples have much higher air bars, which if I understand the argument about accounting statistics, it is because they have overall very low chloride, and that seems to be a curious result, and possibly an explanation buried in it.

So have you guys pursed that or do you intend to pursue that?

DR. PETERMAN: I guess I am a little dense here. I am not sure that I understand. Does anyone want to -- Leonid, do you want to --

DR. CAMPBELL: The air bars at everything about 1250 on the graph on page 6, the original dataset, that all of the high fluoride Chloride 36 samples appear to have significantly larger air bars associated with them than the stuff below your cut-off point.

And that is a curious result. That is not what I would expect for a natural system, unless you have some sort of explanation for why those samples have a low overall chloride.

I understand the accounting statistics

argument, but from just a phenomenological point, why would the high point 36 samples almost uniformly have relatively low amounts of chloride?

DR. PETERMAN: Now, one could speculate. Perhaps it is a function of -- well, there are a number of factors, such as grain size, and how rubblized the sample might be, and leach time, and all of that.

If you look at the slide on page 28, it sort of shows the same thing, and again that is the low concentration values. I mean, this is the validation core, and that doesn't fit the trend that you were talking about in the early Los Alamos data. The lowest concentration values are all less than five or six hundred.

DR. RYAN: And that point is highly uncertain, and that is a whole different interpretation than if it has got a very small error. So uncertainty analysis has got to be factored in to help with the interpretation I think.

DR. PETERMAN: In addition to analytical uncertainty.

MR. ROBECK: I am not too terribly familiar with the issue of the error bars there, but what I am familiar with is the data in the cross-drift. We don't see a good correlation between fluoride concentration and Chloride 36 ratios, at least in the samples with bomb-pulse.

So we don't necessarily see that the highest Chlorine 36 samples have the lowest chloride. They are kind of just scattered all throughout typical chloride ranges.

DR. RYAN: Now, on distribution to understand in detail, because if you can understand that in detail, you can assess some uncertainty on that basis. And if you don't understand that distribution, or have not figured it out from your data yet, that is something that has to be done.

MR. ROBECK: Agreed. I am looking at the dataset from June, and I am puzzled by the reason for those larger air bars with the higher Chlorine 36 values. One thing that comes to mind, and I just throw this out, as I don't know it is in fact the reason here.

But when I do an analysis, I have uncertainty based on internal accounting statistics. I also have an uncertainty that I will assign based on external reproducibility.

Now, that would generally be a percentage. Now, if that is what June has done here, and simply assigned a five percent uncertainty for reproducibility, that those will appear as larger error bars.

DR. RYAN: Again, the basis for that assignment is critical. If it is just a typical measure error is five percent, that's not going to get it.

MR. ROBECK: That would be your internal error based on accounting statistics. It would be based on external reproducibility of standards.

DR. RYAN: You know, I guess my general reaction to the discussion is without a systematic development of uncertainty analysis in the measurements, and all the components, whether it is instrument uncertainty, sampling uncertainty, contaminant uncertainty, and all those things, you really can't interpret these measurements as effectively as you could with the uncertainty.

You know, simple examples like it took a hundred samples of blanks and what is the average measurement. Theoretically, they should all be the same. Well, if they are not, what is the standard deviation.

I mean, something as simple as that gives meaning to how you sample, and 67 percent of the time, you will be within that. I mean, everybody knows those statistics.

And in fact without that laid out on top of an interpretation, it is hard to ascribe meaning to it.

MR. ROBECK: We have analyzed standards, and along with each set of samples, I will send a few standards, which I know the ratio -- and it is a certified ratio, and those ratios come in very good.

DR. RYAN: That is the part that is not going to come out (off microphone).

MR. ROBECK: Right. And let's just not report it here, but it is reported, or at least it will be reported. But, yes, along with blanks that I typically submit, I submit 10 percent of my samples will be standards, and some of them will be spiked standards, and some of them will be unspiked standards, and those results come out quite good.

So the results are reproducible, at least when we have a nice homogeneous sample, and therein lies the problem. It is hard to envision getting a rock that we could claim is homogeneous that we could process 30 times, and then do statistics on our numbers.

DR. RYAN: Again, that is not what the

blanks, and dupes, and all of that are addressed at -- is a fundamental sampling error that -- you know, I think that relates to the steel plate issue, and some of the other things that you have mentioned.

But again quantifying that systematically is critical. If you have not done that failure repeat sample, you should.

DR. PETERMAN: In terms of the samples, there is really attempts to replicate. You know, it was very difficult to replicate results. So if you were to use those duplicates, in a statistical sense the error bars from those would be off the chart.

MR. ROBECK: And that is exactly what we are talking about, and I think that has been the thrust of the early part of this project. We have been exchanging samples, and we did try to prepare what we thought would be a good reference sample, the Evalve-1 sample, and we performed a number of analyses on that.

And lo and behold, it wasn't homogeneous. It is not a straightforward problem to really say, well, here is a homogeneous rock and analyze it 30 times.

CHAIRMAN HORNBERGER: I think the bottom line is that it is a fairly easy problem if your sample or your analysis cost is $10 a sample. And I think that you are probably not doing exactly what Mike wants because your costs are just a little more.

MR. ROBECK: It would be about 40 or 50 samples a year.

DR. RYAN: I appreciate the difficulty (off-microphone).

CHAIRMAN HORNBERGER: Thanks very much. That was very informative, Zell, and we look forward to hearing about your pass forward, and I do think that I really appreciate Abe's answer, because I do think that it is -- well, I would express my belief that it is critical that we do get to the bottom of this.

We don't want to look at this as a puzzling question mark just sitting out there, and I think we can do it. And I think we will come up with a good plan. Thanks very much. We are going to take a break now, and let's take a 25 minute break.

(Whereupon, at 3:18 p.m., the meeting was recessed, and the meeting was resumed at 3:48 p.m.)

CHAIRMAN HORNBERGER: Okay. I would ask everyone to make sure that they have signed in. We would like to keep a record of who attend our meeting.

We are going to continue our presentations on the DOE scientific update, and we are going to hear some of the results on microbial-induced corrosion, and we have a presentation from Joanne Horn. Joanne.

DR. HORN: I just wanted to first thank the committee for giving me the opportunity to present an overview of our program on assessing the impact of microorganisms on long term nuclear waste containment. I think we are ready for the first slide.

Thanks. Mostly our program has been focused on the effects of microorganisms on the waste package, and these are basically categorized as microbiologically influenced corrosion or MIC.

This is really a complex set of interacting microbial facilitated processes, and it includes acid production by bacteria, as well as iron oxidation and reducing reactions, sulfate generation, with a reduction of sulfate, and hydrogen production.

And also the brown kind of bubble there represents what we call biofilm. All these bacteria are embedded in a matrix of polysaccharide, but it is also generated by bacteria.

And the polysaccharide are long chain sugars that produce a kind of slime. The slime prevents the diffusion of oxygen towards the metal surface, and that also produces conditions that can accelerate corrosion.

Now, which of these reactions occur is really dependent on a number of variables, including the environment. That is, for example, that you can't get sulfide generation without having sulfate present, for example.

Also, the organisms that are present and the material under consideration. Next slide, please. So the goals of this program then are to determine the potential for MIC in the Yucca Mountain repository, and determine the conditions under which MIC would occur, and that includes the boundary conditions for microbial growth since we expected initially will start with a sterile environment, at least on the waste package because of the radiation fields generated by the decay of the waste.

But that eventually we did either a reintroduction of bacteria or a regrowth of those organisms that could survive through that radiation field.

Also, the conditions for microbial activities, and so again that would be -- you know, you have to have the necessary sulfates for a given end-product to be generated.

And also the quantified rates of MIC on

the waste package materials, and that would include the production of dilatory and metabolic end products, and also the direct effects on candidate waste package materials. Next slide, please.

Okay. So we have taken this kind of multi-prong approach to answering these questions, and among them ethological studies, and we are looking at the types of organisms that are present, and expected, and that would essentially establish the potential for MIC.

The conditions under which microbial growth would occur, and if you couple that with some of the thermal hydrological testing, for example, you could estimate the time that the MIC might initiate, and that will become clearer on later slides we think.

Looking at the effects of microbial activity on water composition, and so that would be a kind of indirect effect of bacteria or fungi. For example, if they were to acidify the ground water, and then the ground water impacted the waste package.

We need more traditional electrochemical studies to quantify the overall changes and corrosion rates due to MIC, and these studies can also indicate the mechanism by which this acceleration occurs.

We are performing accelerated testing as well, using both mixed cultures, and that is the entire Yucca Mountain community, as well as using pure cultures with defined microbial activities.

And in these studies we are looking at the survecial effects of the materials, and the biochemical effects on water chemistry, and the pure culture studies can provide boundary conditions, and for example, the generation of these deleterious end products. Next slide.

Okay. So first I would just like to just address the ecological studies and we are doing a characterization of the Yucca Mountain microbial and fungi communities, using a number of different methods, and we have also determined what the extant microbial densities in the mountain are, and the growth limiting factors. Next slide, please.

Okay. We started these studies a number of years ago by simply isolating microorganisms from rock that was excavated aseptically from the mountain. This is within the ESF.

And also from the large file test, and what you are seeing on the left there -- and I don't have a pointer, and so I'm sorry. Oh, do we have a pointer in the audience? Wonderful. My hero.

Perfect. So what you said, hopefully it

won't blind you in the process. On the left, you will see -- or laser paint you. Okay. Those are little bits of rock that we actually collected from the repository, aseptically crushed them and aseptically, and what you see there are bacterial colonies growing right out of that.

And those are criteria that are contained on the surface of the rock, and each one of those colonies rises presumably from a single cell. On the left, again, you see bacterial colonies, and those are from actually artificial poured water formulation that we washed this rock with, and then plated that out, and these are all on low nutrient media.

And so you can see that there are indeed bacteria that are contained within the mountain. Next slide, please.

Okay. What we did initially was to first isolate these bacteria, and speciate them, and then we tested them for a number of activities that were associated with corrosion, and found that indeed many of these had these activities.

And so were thereby established the potential for MIC to occur. Next slide, please. We also determined what the bacterial densities in the mountain are, and we did this not by using growth, but by directly extracting fossil lipid fatty acids, which are membrane components, directly from a rock core.

What we did was that we drilled the rock core out of the ESF, and split it in two, and one representing the sort of region that was closest to the drip wall, and one that was further into the wall, and we found that there was some difference in fossil lipid content.

You can estimate the number of bacteria here by normalizing the extracted fossil lipid to that from a known number of bacteria, and you can see that there was some difference between the surface and the at-depth cores.

But the bottom line was that it was about 10 to the 4th, or 10 to the 5^{th} bacteria per gram of dry rock. The next slide.

Okay. We have also done a number of growth studies. This is a graph, and we are looking now just at crushed rock from the site, and amended with -- this is assimilated ground water at 1-X concentration, with or without glucose.

And looking at the growth of bacteria from the rock over time, and what you see is that as soon as you add ground water, you get a significant increase in the numbers of bacteria that you can recover in the acetous phase, up to or from 10 to the 6^{th} bacteria, and approximately without glucose added, or up to 10 to the 8^{th} with glucose.

So this showed us that the major limiting factor to growth was water availability. And as soon as you add water, you are going to get a significant amount of bacterial growth.

And we have also done other studies that I didn't think I would have time to show here, and so I just mention them here. We have also established that phosphate is the major nutrient limiting factor in the mountain, and that if you actually add phosphate back to these systems, you get an increase on the order of one to two orders of magnitude. And carbon is well as this slide shows.

There is apparently enough sulfate and nitrate in the mountain to support growth, even in 1-X ground water. Next slide, please. Now, this is important because it tells us when the possible kind of on-switch for bacterial effects would occur during a repository revolution.

And I just want to apologize here for the slide. I think we lost a little in the transport of these slides from Livermore to here, but on the left is relative humidity, and this is actually down from

Tom Bucheff's modeling group at Livermore, the thermal hydrology.

And what we are looking at is the relative humidity on the rock walls over time after closure. Okay. So this would be after ventilation is shut off, and what we see here is that it areas of low infiltration, the dose of humidity never increase over 70 percent.

But in areas of higher infiltration and I think that is about 50 millimeters per year, you almost maintain a hundred percent humidity on the rock walls.

So knowing that water is a major limiting factor for growth, we could see that in areas of high infiltration, you will have growth supported almost immediately after closure.

Whereas, in areas of low infiltration, you may never reach the humidity's that are required for growth, and actually in the models we have put the cut-off for bacterial growth at 90 percent humidity, which is probably conservative.

The literature is more on the order of 95 percent. Okay. Next slide.

CHAIRMAN HORNBERGER: Joanne, can I --

DR. HORN: Sure.

CHAIRMAN HORNBERGER: Going back to your previous slide, where you concluded that water is the major limiting factor, what are they growing on? I assume that these are aerobic experiments?

DR. HORN: Yes. These are aerobic experiments.

CHAIRMAN HORNBERGER: And what is the energy source?

DR. HORN: You know, we -- I don't know whether it is dead cells, and if you look in the literature, there is some evidence in the deed subsurface, things like organic carbon being a possible source.

Some of these organisms do fix CO2, and so not all of them need an organic carbon source. You know, we have isolated all the bacteria that we could out of those experiments, and indeed we have found some CO2 fixers.

MS. TREICHEL: What is the numbers on the bottom?

DR. HORN: Maybe we should -- oh, I'm sorry, on this slide?

MS. TREICHEL: Yes.

DR. HORN: I think it starts at 150 years, because I think that's when closure starts. And I think those are a hundred year increments.

MS. TREICHEL: And 450 and -

DR. HORN: Right.

MR. LEVENSON: Joanne, on your slide that George just asked about, where you have the sterile control. What was the water and glucose, or what was the sterile control with water, and with glucose, or without glucose?

DR. HORN: The sterile control actually simply contains rock that was sterilized. What we do to sterilize the rock is that it is actually fairly typical to sterile Yucca Mountain rock.

We have tried autoclave emitter periodically and that doesn't work. We use a gamma or a cobalt-60 source, and we eradiate it for about at least three mega-reds.

MR. LEVENSON: And what was the media? Was it in water or in --

DR. HORN: Yes. It was, and so you then have to sterilize rock or non-sterilize rock, and we added a formulation that approximated Delaney's formulation for J-13.

I can show you that. I actually brought some extra overheads. You know, I apologize, because I thought that I would have a little less time than I did. So I kind of eliminated some things. But if you are interested -- well, okay, next slide please.

Okay. When you grow organisms from any environmental sample for that matter, you only recover about one percent of those organisms that are present. So to overcome that, there has been methods developed to directly extracting DNA from environmental materials, and they characterize in the organisms by sequencing the DNA that has been extracted.

And we have actually done a study on Yucca Mountain rock, so that we kind of like brought out a stone, and we got DNA out of rock. It took about half a kilo of rock to extract a sufficient amount, but we were able through various biochemical and genetic manipulations to separate these DNAs, and to take the unique ones, and have them sequenced.

And what this is, is to follow the genetic or evolutionary tree of the organisms that we were able to identify, using this DNA analysis. And we recovered about -- well, we identified about 65 different organisms.

And you can see that they stand out -- these are actually about 45 of them, and we have 20 of them that we still need to actually insert into the tree.

And then you can see that they span over a broad and follow a genetic range, and they include these high GC gram-positive organisms that are typically found in betas one areas, and they are very resistant to desiccation, and a number of other organisms.

These proteobacteria are very metabolically diverse, and a lot of them produce acids, and have different metabolisms that are in fact associated with corrosion.

So this is really meant to give us a kind of baseline, although the repository is expected to be an open system and so anything that we presume is going to be able to invade and get in there. But at least we will know what we are starting off with.

And if we associate the metabolic activities with their ability to produce corrosion of these various groups of bacteria, we may be able to get a handle on at least what we will be dealing with. Next slide, please.

We also looked at or identified a number of different fungi and we have identified these. These were actually obtained by slotting and just growing and isolating various bacteria from a region of the ESF where there ventilation had been shut off.

And so fungi are important or potentially important because they produce organic acids, and the waste package materials could be susceptible to these production of bioorganic acids. Next slide, please.

We also have done some experiments or we are actually in the process of doing these now, but we have a long-term corrosion experiment that is going on at Livermore, and this is depicted here. This is a picture of the facility.

Each one of these tanks is about a thousand liters and they contain -- they are actually environments that mimic the expected repository environment over time.

They vary in ironic strength, and temperature, and pH, and although no bacteria was introduced intentionally into these tanks initially, we had preliminary evidence that at least some of the tanks had been at least somewhat colonized.

But what is interesting to us about this is that it sort of reflects the repository evolution. That is, that you start off with a fairly sterile environment, and then kind of anything that is wrong that can survive in there will do so.

And so we thought that it would be a good thing to test these tanks and analogously determine what the microbial sort of roster of organisms is in there to see what may fly into the repository and survive.

Okay. Next slide. So this is the results from one of these tanks. This is a tank that contains water that is meant to mimic dilute ground water at 60 degrees, and it contains the corrosion resistant materials like Alloy-22.

And we found five different groups of organisms I should say, and we actually had an organism that is radiation resistant interestingly enough, and we also found one that was heat tolerant, and then the bacilli, there were five different bacilli that we isolated that were identified that were actually all sporulating organisms that came in with both desiccation and high temperature.

And we are analyzing another tank now that is acidified water at 60 degrees, and from that we have observed a very strong DNA signal, and we have cloned, or amplified and cloned the DNA, and we are screening them now to determine which organisms are present.

MR. LEVENSON: Joanne, excuse me, but on your slide that shows the facility, does the tank environments mimic expected repository environments?

These tanks all have liquid phases?

DR. HORN: They both -- actually they are half full, and so they have half of the samples of the corrosion coupons are actually submerged and then half of them are in the vapor phase.

MR. LEVENSON: It may be that a possible repository environment would be better than expected. It is full of water.

DR. HORN: Right. This is true. I guess mostly the chemistry was what we were most concerned about when devising the environment that was being tested. But thanks, Milt, you're right. Next slide.

So just a summary then of some of these ecological growth studies. We know that microorganisms are extant in Yucca Mountain rock, to the density of about 10 to the 4^{th}, to 10 to the 5^{th} bacteria per gram.

There is also a wide variety of fungi, and the major growth limiting factor appears to be water, and when water becomes available, we will expect that microbial growth will ensue.

That also we are expecting that infiltrating water will likely transport organisms into the repository, and cultured Yucca Mountain bacteria have activities associated with MIC, and this establishes a potential for MIC in the repository.

That uncultured identified organisms span a wide phylogenetic range, and their activities are being investigated for MIC activities. In the investigations of the corrosion test tanks, show that organisms adapted to repository environments will become established.

Okay. Next slide. So I would like to move on then to electrochemical studies that we have conducted to quantify the overall contribution of microorganisms to corrosion, and then these types of studies also offer an indication of the mechanism of biogenic alterations to corrosion rates. Next slide.

So primarily for the studies thus far, we have used a test cell that we have actually devised at Livermore and this is composed of -- on the bottom of this working coupon is the material that is being tested, and it forms the base of the vessel.

And we either cook these with Yucca Mountain microorganisms that we have isolated and characterized, or we leave it sterile. So we consistently try to run our experiments under both sterile and non-sterile conditions to determine what the biotic effects are. So you can subtract out all the biotic effects.

And the media that we have used in these experiments again thus far is a fairly rich media and sort of accelerated the whole process and produced microbial growth.

And into this we have a platinum electrode that is attached actually to potentiasac (phonetic), and under an applied current, you can build up a potential on this coupon and compare it to that of a reference electrode, and it turns out that the corrosion potential or the potential build-up is directly correlated to corrosion rates.

So this is a means of actually measuring corrosion rates in real time. Okay. The next slide. So we incubate these for a period of -- in this case up to about five mines, and this is looking at -- and, you know, I apologize, because when they reproduced these overheads in black and white, I think you lost like the green like the green and the red, and you can't decipher.

But what this depicts is one of these linear polarization studies with either carbon steel, or Alloy-400, which is a copper nickel alloy. You can see that under sterile conditions this is the Alloy-400, a fairly corrosion resistant material.

Notice here that corrosion rates and

microns per year are on a log scale, and so you have very low corrosion rates under sterile conditions. And yet when you add the bacteria, and that is the red circles, and they appear, you can see that you have increased corrosion rates on the order of 200-fold.

Similarly, with carbon steel, under sterile controls, and that is the yellow squares, you have a lower corrosion rate, albeit it's on the order of one micron per year.

And it increases to about 8-fold, or I think it is about 6-to-8-fold actually when you add bacteria to the system. So in this way we are able to actually establish what we call an MIC factor, or that factor by which microorganisms increase the corrosion rate of a given material.

And in this case, it increases the rates of Alloy-400 almost to sterile, the level of the sterile carbon steel. Next slide, please.

Okay. This is the same type of study, and this time we are looking at probably most interestingly Alloy-22 and stainless steel 304, as well as I625, and what you see is the Alloy-22 under sterile conditions, and non-sterile.

And you will notice here on the Y-access that these corrosion rates are much lower than that of the Alloy-22, or I'm sorry, the Alloy-400 or carbon steel. This being one of the reasons that we are using Alloy 22, or not using it, but promoting it as a possible candidate material for the corrosion resistant barrier of the waste package.

And the bacteria, at least in this experiment, don't appear to have that much of an effect. I mean, they raise it by the order of two-fold, and they have actually incorporated that MIC factor into the current models, and the next slide -- oh, I'm sorry.

So at the termination of these experiments, we did what was called an anodic polarization test, and what this shows is three of these materials, and again a sterile control, and inoculated with Yucca Mountain bacteria.

And you can see that under a given potential here that there is always a higher current density with the Yucca Mountain bacteria, and this is fairly consistent for altering materials.

This actually shows that the mechanism by which these bacteria are causing these increased corrosion rates is by accelerating the anodic reaction or the dissolution of metal.

So we think that is how they are working, and we are investigating that further. The next slide is sort of a summation of the status. Again, for example, the carbon steel, these are average corrosion rates, and so what we have done is just under steady state averaged all those points.

Again, a factor of about 6 or 7-fold under sterile conditions, versus non-sterile, and then again for Alloy-22, only by a two-fold difference. Now, this may be somewhat of an under-estimate of corrosion rates, because if you recall when I showed you the set-up of this experiment, it is actually run under batch conditions for about five months, and although we have not measured the oxygen concentrations, we think they are going anaerobic. That would be fairly typical. They are not being mixed or aerated.

And so we would expect that they would be depressed or the overall corrosion rates. Now, the actual MIC factor or ratio of sterile to non-sterile may remain the same. But we are checking that out by running these experiments under aerated conditions presently. Okay. Next slide.

I don't want to make too much of this, because this is a very preliminary result, but what we did was to test at the termination when we tore down some of these experiments what the soluablized concentration of alloy elements were in solution to see if we could get any idea of how fast the metal was going away, or which alloy elements might appear.

And what we saw in the case of Alloy-22 was that when it was sterile, we couldn't detect either nickel or chrome in solutions, but when we added the bacteria, we detected a noticeable amount of chrome. This is in parts per million.

Now, this isn't to say that we are actually getting selected dissolution of alloy elements. It may be that everything does go away at the same time, but that some of the alloy elements reprecipitate.

So I don't want to make too much of this, but what we are doing now is to -- that instead of looking at what is left in the solution, we are looking at what is left on the coupon, okay? And that is a much better measure, using sputtering x-ray photoelectron spectrostrophy, we can actually determine what the ratio of alloy elements is as we sputter into the metal on a very high resolution.

So it is a much better measure of what is going on with the mode of dissolution here. Okay. Next slide.

So to summarize then these electrochemical and dissolution studies, carbon steel shows an increase in corrosion rates for the Yucca Mountain bacteria and Monel shows even a greater MIC factor.

The Alloy 22 shows a lower increase in MIC factor, only two-fold so far, and delineated MIC factors require further investigations under more representative, i.e., aerobic conditions.

And this is another aspect that I neglected to mention, was that when you polarization this, normally you use to measure a generalized rates of corrosion, and MIC is usually characterized by what is called a localized effect.

That is, it is more associated with pitting and so forth. Now a better way to assess that is using cyclic polarizations. So what we are doing now, is that we have got some testing planned to better estimate these localized corrosion effects.

Okay. To date, the anodic polarization analyses demonstrate that microbes are causing an increase in anodic activity; that is, metal dissolution.

And that the MIC factors thus far determined have been incorporated into a role model. The next slide. Okay. Let me move on to our accelerated materials testing program, and we are actually doing three different types of testing for this.

We have got a simulated saturated repository environment that we call microcosm for obvious reasons, although Milt may disagree, and then we are doing peer culture studies and using organisms with defined microbial activities.

And we are also doing some batch chemical testing. So I will describe each one of these. Next slide. These are simulated saturated repository microcosms. They are fairly simple systems, but they include what we expect would be all the elements of a saturated repository.

So what we do is we feed the actual microcosm environment with a formulation that is ten-fold concentration of J13 ground water. We supplement it with some glucose to accelerate the process, the microbial growth, and we feed this at a very slow rate, at about 2 mils an hour, into this vessel, which contains aseptically collected and crushed rock.

And again we run these under both sterile and non-sterile conditions. Again, sterile controls are produced by eradiating the rock at 3 mega rads. And into this we also put candidate material coupons of waste package materials.

So that periodically we can withdraw the coupons and look at the surfacial effects of the bacterium. Next slide. This is just a picture of what some of these microcosms setups look at. This is the reservoirs, and these are being incubated at 30 degrees C.

We were running them presently at 30 and at room temperature, and it goes through a pump into the microcosms and out through a pump and into a waste reservoir. Next slide.

And one of the things that we have been able to do is that we when we withdraw coupons, we look at them first just under fixed, and we fix them with either glutaalgahyde (phonetic) or we approximate a critical point fixing.

But if corrosion products are evident, we can image them using scanning electronscopy. And then in this case it is carbon steel, and so the corrosion products build up rather quickly and these are just different mil basis that we have been able to identify through facial chemical effects. Next slide.

An in fact this is just looking at the SEM, and we can identify the morphology of these corrosion products, and we are using the EDS, and we can identify their elemental makeup, and we have been able to do x-ray refraction and actually identify the mineral phases.

So we can match these up, and pretty much not only identify the mineral phases, but what likely they originate from. For example, the silica in this case comes from the rock that we have incorporated into the system. Next slide.

Now, despite the fact that these systems are being fed continuously, and you are continuously getting a dilution of whatever chemical effects are occurring in that microcosm.

And you are also washing out any of the chemical alterations. We have been able to detect and I don't want to make too much of this either, because you are looking at parts per billion here, but this molybdenum in the efflux, that is, in the angelus phase of a microcosm containing Alloy 22, and under non-sterile conditions at 30 degrees.

And we really are not seeing the same thing with the sterile controls or the new metal controls, or even the non-sterile at room temperature. But again we are investigating this further. Next slide.

Okay. When we withdraw these coupons, as

I said, first we fix them and image them, and then we clean them. And we use high resolution imaging techniques and in this case atomic force microscopy, to look at the surface and to see if we can discern any differences due to the presence of bacteria in the rock.

And here you see that this is what we start off with. The surface was sanded to 600 grid initially, and so it is fairly rough, and that is what these striations are. Again, I want to emphasize that you are looking at a very small piece of property here. This is a hundred-square microns, okay?

And the Z-axis is 3 or 3-1/2 microns, okay? So it is a very high resolution. The sterile controls for microcosms containing just the sterilized rock, you see a kind of flattening of the striations.

And in the non-sterile coupons, these are all incubated for a year, and the non-sterile coupon, you can see that there appears to be a kind of redistribution of the roughness, and it may be something like nano to micropitting. The problem here I think with this analysis is you are starting with a rough surface, and you are ending with a rough surface.

So it is pretty darn difficult to get your arms around quantitatively around what is happening. So what we are doing now to remedy that situation is to incubate mirror finish coupons. So that means that we start with a much flatter surface, and if we see it erupting, we can at least quantify it.

Next slide. Okay. This is looking at a non-sterile coupon of Alloy-22 going through two years in a non-sterile microcosm, and 1 year or 1-1/2 years, 2 years. So again there does seem to be some effect, but they are small.

Again, the Z-axis is 3 microns, but they are clearly not rare events. I mean, we can zero in on these regions without too much difficulty. But we need to get a better handle on the distribution of these events as well. Next slide.

So to summarize the microcosm experiments then, we have got a system that allows analysis of material effects in an environment that includes essential elements of a repository. That the effects of the microorganisms can be discerned by comparison with a biotic controls. And we also have no metal controls, and so we can look at the effects of the rock top.

We have combined chemical, analytical, and imaging techniques to quantify specie and corrosion products. We also do gravimetric analysis of these materials, which permits the estimation of corrosion rates and effects.

And there appears to be some nano effects of microbial activity on Alloy 22, but quantification and distribution of corrosion needs to be analyzed with mirror finish coupons, and then the results can be incorporated into the corrosion models.

Next slide. So we are also doing some pure culture work, and what I did was to go through a kind of systematic analysis of Alloy-22 and titanium primarily may most likely be susceptible to microbial corrosion.

And what I came up with is -- and then what we did was to pick organisms that have these specific activities, and grow them in peer culture. So this is what we call a microbiology continuous culture.

So you are constantly feeding the bacteria, and grow them under optimal growth conditions, okay? So what we are doing is producing this very vigorous high-density culture, and then we picked these specific bacterium. Clostridium produces hydrogen at point high rates.

And in order to see if they generate

hydrogen embrittlement, and we are also testing a sulfate reducing bacteria that produces sulfide, and it also happens to grow in high salt environment.

We are looking at a thiobacillus organism that generates sulfuric acid when grown in reduced sulfur medium, and we also are taking a mixture of Yucca Mountain fungi that we isolated, and we are growing that in some rich broth to see if the generation of organic acids is going to affect corrosion of these materials. The next slide.

So this is the microcosms, except that now we have just -- we don't have any rock in these studies, but rather we have these defined organisms in separate experiments, and they are being fed with media that is conducive to generating these possibly deleterious end products, and in the reactors we have got trays, Teflon trays containing both titanium Grade 7 and Alloy 22.

And of course they are being drained at the same rate that they are being fed at. The next slide.

This is just a picture of a c. acetobutylicum bioreactor. It is about a one liter vessel, and this is actually contained in the anaerobic glove box, because these are anaerobic organisms that are grown in an nitrogen atmosphere. Next slide.

Okay. This is a picture or an SEM image I should say of the biofilm formation on a titanium coupon in a sulfate reducing bioreactor. And you can see that on the little rods here that they are microorganisms. They are colonizing the surface, and on the right this is actually a picture of or an image of that polysaccharide matrix that I mentioned earlier.

And you have to of course dry the samples, fix and dry them in order to see them in the SEM, and so when you dry them, the film tends to crack them, and that is what you are seeing there. But it is definitely evident and present. And the next slide.

So this is the sulfuric acid producing culture, and after seven months we withdrew some of the coupons, and surprisingly we actually found some dissolution of titanium from the surface.

This is again what we started with, AFM images again. This is in a sterile control and that is just incubated in a bisulfate medium. And it looks fairly degraded when we looked at the same, or the one that was exposed to culture.

And we actually found that we precipitated the titanium in the reactor, and the increasing roughing was also confirmed by doing what is called a root mean square analysis. Root mean squares are an index of surface roughness, and you can see here with the titanium that you actually increase the surface roughness.

But fortunately with the Alloy-22 that it didn't seem to have any effect. So that was a good thing. But this isn't actually the first report of MIC of titanium. People have looked for it for a long time, but they never used quite these conditions. The next slide.

So the summary of our pure culture studies so far is that we can analyze the effects of specific deleterious metabolic products on material performance, and it permits the determination of the upper limits of generation of these end products.

We are actually establishing that now, and we are doing things like measuring the organic acid concentrations of several organic acids, including those that have been recently found by the USGS in pour water form the site.

And it establishes some kind of upper bound so that we can incorporate those into models for the production of these end-products. And despite the fact that there is a continual input and output into the system, a steady date is gained, and I didn't really show this slide, but we have been able to see, for example, titanium dissolution again in our clostridium CW2 reactor.

So we can actually see a surfacial analysis of the material coupons is now ongoing. Okay. The next slide. I just lastly wanted to mention a set of experiments that we have just recently initiated, and I wanted to get past this dissolution and washing sort of issue that is connected with continual flow systems.

What we have done is to start some experiments under batch conditions so that we can look at the build up or accumulation of either alloy elements if they are being soluablized or of the metabolic or alterations to ground water that the microorganisms are generating.

And so in these experiments, we are using crushed tuff and our simulated J-13 ground water, and we can use either anaerobic or aerobic atmospheres.

And we think that we are actually using Alloy-22 foil and the reason that we are doing that is to sort of increase the surface area and the mass ratio. So that if these materials are actually being corroded, we can detect them more readily by just having more surface area being exposed in the experiment.

And we can -- and, of course, we always run our sterile controls with or without organisms. We are also running with them without a carbon source. And we are analyzing periodically the generation of sulfide acids with a waste package alloy elements.

So it is sort of looking at all these different alloytes so that we can get a better picture of what the change in chemistry is both for the alloy that we are testing, as well as the ground water. The next slide.

So just to summarize overall then of our MIC studies to date, we are looking at the potential for MIC to occur, and that has been affirmatively determined.

We are looking at the conditions for microbial growth, which have been established, and then coupled with thermo hydrological modeling, and this establishes when MIC may become a factor for microbial effects.

We have generated a roster of organisms extant at the Yucca Mountain site and also organisms that may colonize the repository. And then if we --

and answering that why question, coupled with their associated metabolic activities, this information will allow what MIC activities may be relevant to waste package corrosion.

And initial MIC factors have been determined, and establishing the overall contribution of microorganisms to waste package corrosion, and we are doing further testing on that, and under other conditions.

Our dissolution rates and corrosion modes of engineered barrier materials are being determined, and the upper limits of deleterious bacterial end products and their effects on these materials are being established.

And lastly the effects of the Yucca Mountain groundwater are currently under investigation. So with that, I will conclude my presentation and invite any questions from the panel.

CHAIRMAN HORNBERGER: Thank you, Joanne. Milt, as our MIC expert, would you like to go first? Well, Ray, do you have any questions?

VICE CHAIRMAN WYMER: First, let me say that it looks like a very nice work, and it is a lot more than I have seen up to this time, and you are to be congratulated on the scope of your studies, because they are very broad in trying to cover all the parameters of interest.

DR. HORN: Thank you.

VICE CHAIRMAN WYMER: I do have some questions that you probably have not had enough time to do research on to answer yet, but let me go ahead and fire away.

First, I wondered about the potential rate of bacteria entering the repository by whatever route that they enter over a long period of time, and whether there is enough there that it makes any difference.

DR. HORN: Well, you know, it doesn't take very much to start with to generate a lot, because they divide by binary fusion. So they grow at an incredibly high rate if the conditions are right.

VICE CHAIRMAN WYNER: If the nutrients are there?

DR. HORN: Yes, and I think -- well, pretty much the assumption is at this point in the field is that organisms in the deep subsurface primarily are -- and they either originate when the rock was laid down, or they infiltrate with incoming ground water.

So in this case, we would be looking at

infiltrations. So I think that the number of microorganisms that come in absent ventilation, but that is another issue, will be primarily dependent upon infiltration items.

VICE CHAIRMAN WYMER: And I suppose the nutrients have to come in with them?

DR. HORN: Yes, except that so far we have found that they don't need very much to grow. If we give them ground water, even unamated within a carbon source, that they appear to be able to pick up and grow fairly readily.

VICE CHAIRMAN WYMER: Of course, they all need a phosphate backbone, and so --

DR. HORN: That's true, that is an essential element. Now, there is a about 200 ppm phosphate in the rock, which I am sure that many of you are aware of. And when we don't put -- and I didn't show these experiments, but when you don't add phosphate to rock, we are presuming that the phosphate that they are growing on, they are dissolving from rock. And there is actually a good deal of evidence in the literature to suggest that bacteria can readily dissolve phosphate from the rock.

VICE CHAIRMAN WYMER: Okay. There is a question of the mixtures of the bacteria comes up, and you did studies with typical Yucca Mountain mixtures of bacteria.

DR. HORN: Right.

VICE CHAIRMAN WYMER: But then you are doing the peer culture studies, too.

DR. HORN: Right.

VICE CHAIRMAN WYMER: It looks to me like some of these bacteria would be fighting each other, and they are reducing bacteria, and they are oxidizing bacteria.

DR. HORN: Yes. Yes. And that occurs in subsurface environments. As an example, there are methane producing bacteria that attack CO2 and reduce it to methane, and then there are methane oxidizing bacteria that use the methane as a carbon source and generate CO2. So, analogously, you know, manganese oxidizers.

VICE CHAIRMAN WYMER: And in the repository the question is who wins?

DR. HORN: Well, actually, in this case I don't really think that they are fighting each other, because in a way they are really facilitating each other's physiology. In other words, if you are a manganese oxidizer, you need reduced manganese, and so if you have a manganese reducer that is producing that as an energy source for the manganese oxidizer, that guy kind of has it made.

So I think in some sense that if you look at the overall storic-metrics, as a chemist, I can understand how you think. But from a microorganisms point of view, this is a good thing, because you have got available sub-stain.

VICE CHAIRMAN WYMER: And you have, for example, that you are either making sulfite, or you are making sulfate?

DR. HORN: Right.

VICE CHAIRMAN WYMER: You are not making both of them.

DR. HORN: Yes, the sulfite oxidizing bacteria are actually anaerobic. And ultimately these things are striated according to their environmental micro-niche.

So, for example, the sulfite generating bacteria are anaerobic. And then you see this, for example, in sediments in marine sediments, where you have a lot of sulfate and sea water, and you have got a lot of sulfate generating bacteria in sea water. But you get right into the sediment and then you get very anaerobic. You only have to get down a couple of millimeters and then you get sulfite generation.

VICE CHAIRMAN WYMER: In a waste package, you are probably going to have one or the other.

DR. HORN: Well, in even that, in these binner films, you have very diverse microenvironments. So, for example, at the top, you can have an oxidizing environment, and then the oxygen concentration pops precipitously as you go towards a metal surface.

And so you can have these sort of micro-niches, where things that have very diverse physiologies can actually exist side by side. So I know that it is sort of counter-intuitive, but apparently that has been shown.

VICE CHAIRMAN WYMER: Actually, I have argued in the past for reducing environment, and what is the repository in localized areas which supports the oxidation.

DR. HORN: Yes, and from a micro logical point of view, everything runs slower under anaerobic conditions. You just don't get as much energy out of the anaerobic metabolism. And so from that point of view, I think an anaerobic reducing environment is sort of better.

VICE CHAIRMAN WYMER: Now, what about temperature effects? How do these --

DR. HORN: Sure, superimpose them.

VICE CHAIRMAN WYNER: Are you planning experiments at several temperatures?

DR. HORN: Yes, we found some sort of crude kind of -- well, just kind of under anaerobic conditions moving the temperature up. We have not found much growth after about 60 degrees, but just the organisms that are extant in the rock.

Of course, we know that there are organisms --you know, those that grow in hot springs and down in the smoken vents in the deep sea that can exist up to temperatures -- I think about the upper limit for life is about 120 degrees C.

We are not sure whether we are seeing any of those organisms. So far we haven't found any. We are still at the beginning of testing the tanks, and that is one of the reasons that I think those test environments are going to be really interesting to see, and if there are any floating around, are they going to become established there.

Because the canonical thought in environmental biology is that things will grow and become established if they are adaptive to a particular environment.

So it is not totally beyond the realm of possibility that we will see these things growing and

--

VICE CHAIRMAN WYMER: And it could be quite a while before the surface of the waste package will get down to 60 degrees or 70 degrees.

DR. HORN: And even more than temperature, I think it is going to be water availability, because we know that there are things that can grow at high temperature. But water availabilities -- I mean, life needs water, and that is the bottom line.

And so we really are not expecting microbial growth until water reenters the repository, but the water availability is tied directly to the temperature of radiation. So as the temperature drops, water increases, and radiation drops.

So those three factors are really tied together, but since water seems to be the primary riveting factor, we have kind of picked on that as the kind of switch.

VICE CHAIRMAN WYNER: And on the waste package, you do have both temperature and radiation fighting you pretty good?

DR. HORN: Right. Absolutely, and those things will prevent the growth directly on the waste package for thank god a good long length of time.

VICE CHAIRMAN WYMER: John Garrick has

given me permission to ask his so what question.

DR. HORN: Yes, so what, and I was expecting that.

VICE CHAIRMAN WYNER: Just take your general corrosion rates from one of your viewgraphs, and you come up with maybe for the Alloy-22 a couple of millimeters in 10,000 years, and for stainless steel, 3 or 4, or maybe twice that.

DR. HORN: Right.

VICE CHAIRMAN WYMER: Maybe for 3 or 4 millimeters, maybe 10,000 years.

DR. HORN: Right.

VICE CHAIRMAN WYMER: That doesn't get through the waste package. So let me ask you what is your opinion about the significance of the microbial on the waste package?

DR. HORN: Well, you know, I mean, we didn't design these experiments to prove that bacteria were going to be a problem. We designed them to answer that question will they be a problem.

So I think under the conditions of this particular experiment, we have shown that it won't be a problem, which is a good thing. Now, like I said, these may be depressed somewhat because of the conditions under which we ran these experiments, and that's why we are repeating them.

And we are also doing some alternative types of testing that are better at looking at sort of localized pitting, which is what bacteria are really known to do.

VICE CHAIRMAN WYMER: Well, thank you very much. That is really nice work.

DR. HORN: Thank you.

DR. GARRICK: Just continuing with that a little bit. I am curious about how much microbial corrosion you would have to have in order for that as a waste package integrity threatening mechanism to be competitive with, for example, the current corrosion model, which is a diffusive transport model that eventually leads to intergranular corrosion cracks in the absence of water, and only in the presence of an assumption about a film.

So there is no water until the drip shield begins to fail, which according to the current model doesn't occur for several tens of thousands of years.

So what is the relevance of all of that? If you have already got a failed waste package in the absence of water, how can we become concerned about a contribution that comes from a phenomena that has to be in the presence of water?

DR. HORN: So you mean that you can't kill it twice?

DR. GARRICK: Yes.

DR. HORN: You know, I might just defer to one of my colleagues who has more familiarity with some of the other modes of corrosion. Dan McCrite has been in the program for a long time, and Dan, do you want to give that one a crack?

MR. MCCRITE: Well, one of our major concerns with the MIC factor is what it would do to localized corrosion, and possibly stress-corrosion cracking, again in an anaeceous setting, because in those cases the MIC factor would be a lot more than just two. It would be in the thousands.

And that is analogous to some of the industrial or field studies that components have failed by MIC components, particularly the stainless types of materials, like stainless steel and so forth.

But when MIC is a significant factor in your corrosion, it is usually in a crevice or around the weld. And today we have not studied all those things with MIC as also a component. We have done a lot of testing in just purely a biotic condition, but we plan to also do those same kinds of studies with MIC components.

It is a little harder test to do because obviously we have to keep the microbes alive during the duration of the experiment. So also have had some problems in getting suitable samples, especially welding samples, where we will carry those experiments out.

So the data has been essentially the effect of MIC on general corrosion, which really isn't much of a major problem with Alloy C-22, whether it is biotic or a biotic. But we think that if there is an effect that that it is going to be in localized corrosion and stress corrosion cracking, and those experiments remain to be done, particularly with MIC as a component.

DR. HORN: And just to add a little bit to that, is that it has been established that microorganisms really like weldments, and so we are pretty anxious do these same experiments and look at the differential effects on weldments.

DR. GARRICK: Many years ago, when the WHIP project was going through a stage similar to what the Yucca Mount Project is going through now, one of the big worries was gas generation.

And one of the big anxieties about gas generation, at least in the early days, was microbial induced corrosion on the drugs, et cetera, et cetera.

Eventually that issue seemed to go away, and the experts on microbial corrosion came forward and essentially indicated that this was not a real issue.

Is the information that led to that conclusion or the technology that was associated with that effort -- and I realize that geology is very different, and the materials are very different, except for iron. But has that information been a part of your --

DR. HORN: You know, we have not worried about it too much, because we really have an open system here. I mean, are you talking about within waste packages?

DR. GARRICK: Yes.

DR. HORN: Well, I am not too worried about within waste packages, because I think everything is just going to be killed there, and the wooden facility, since it is a low level radiation environment, they were much more susceptible I think.

So once bacteria can recolonize the inside of a waste package, it has already been breached, and so already you have defined an open system. And we know that it is not like in the Canadian version or their design.

It is a very tightly packed system. I know that they are also worried about gas generation and pressure buildup, but I think the inside of the packages are going to be sterile. If anything ever gets in there to recolonize, by definition it has to be breached.

So you don't have to worry about pressure build up on the inside of the cans. And then on the exterior of the packages, I don't think we have to worry about pressure buildup, because we essentially have a breathing open system.

DR. GARRICK: I wasn't thinking of it so much as having to worry about pressure buildup. I was more thinking about it at the mechanistic level, and the mechanisms.

DR. HORN: Well, we have this one experiment going right now, and I guess hydrogen embrittlement is more of a concern for titanium, and so we have got this hydrogen producing culture that generates hydrogen like nobody's business.

And so we are actually testing whether we can induce hydrogen embrittlement by these organisms.

It is kind of a worse-case scenario, and then looking at the mechanical effects, and we will be doing the same on the surface to see if there is actual hydrogen invasion as a result of microbial generation of hydrogen.

So from the literature is real ambiguous on this topic. Nobody has ever definitely seen MIC induced hydrogen embrittlement.

DR. GARRICK: And just a final comment. While you are doing these experiments are you also thinking in terms of possible methods of mitigating microbial corrosion?

DR. HORN: You know, I think that was sort of -- you know, because anything would have to be a kind of engineered approach, and I think everybody is very hesitant to -- you know, for example, I think somebody really early on suggested, well, why don't you add a micro side, and I think over a 10,000 year period that everybody is fairly convinced that just is not a practical approach.

So what we are doing is trying to rely on the materials to resist corrosion, rather than trying to get rid of the bacteria.

DR. GARRICK: Okay. Thank you.

CHAIRMAN HORNBERGER: Joanna, I am still -

- I am interested in how the results actually get scaled to the repository, and again in this sense, I asked you the question about the source of energy to run this system.

And you replied, well, it could be on a chemoanotropic base.

DR. HORN: Right.

CHAIRMAN HORNBERGER: Or it had to come in with the water. In either case it strikes me that the 10 to the 4^{th} and 10 to the 5^{th} bacteria per gram of rock is not a big thick biofilm.

DR. HORN: Right.

CHAIRMAN HORNBERGER: And I can't see that you are going to bring an energy source in with the waste package.

DR. HORN: I guess the thing that concerns me is that when you do add that ground water, even without a carbon source, you see up to 10 to the 8^{th} bacteria, and that is actually per ml.

That is actually the platonic bacteria that are floating around in the aqueous phase, and bacteria like to stick to things. So it is at least that many, and there is probably more stuck to the rock.

DR. HORNBERGER: Then why do you only

measure 10 to the 4^{th} and 10 to the 5^{th} in the rock?

DR. HORN: Because you don't have water there now, okay? So right now there is 10 to the 4^{th} to 10 to the 5^{th}, but they are looking at perturbing the system and we are going to drive the water away presumably and then it is going to come back.

And I think the infiltration rates are going to be what determines the microbial growth.

CHAIRMAN HORNBERGER: So basically you are looking at this as a potential problem in the superfluvial, where the infiltration rates are higher?

DR. HORN: Precisely.

MR. LEVENSON: One of the things that I have been asking about I can't seem to get an answer, as to why with the present design the inner-container is stainless steel instead of just iron or carbon steel, from just the standpoint of microbial corrosion, or microbial enhanced corrosion.

Is there any advantage to stainless steel, as opposed to ordinary steel?

DR. HORN: Well, right now we are really not taking any credit for the inner-package. It is just as a structural support for the outer package.

MR. LEVENSON: I know that they are not taking any credit, but as a taxpayer, I am paying for it.

DR. HORN: Yes. Well, I think I am going to call on Dan for this because he has been around the carbon steel days, and has more of a justification for the switch.

MR. MCCRITE: Just arguing from general corrosion to stainless steel, the general corrosion rate will be under almost any circumstance will be less than carbon steel.

So one of the reasons for picking stainless steel for the inner-barrier than carbon steel was that if and when the outer barrier is breached, if it were stainless steel, it would corrode still much the same way as the Alloy-22 did by some localized mechanism.

If it is carbon steel, it will corrode much more vigorously, and probably with some volumetric change, and so in which case the whole package would stand to rupture open, and more so if it were a more corrosion resistant material inside.

So again our concept of the corroded waste package is that we would never have lots and lots of area exposed, and that it would be just crack by crack and tit for tit. It would be a very small, small amount of actual area that was corroded through and where the water could penetrate through, rather than a very large area.

So we thought that the stainless steel inside would help in that argument.

MR. LEVENSON: But the argument that you are not making, since you are taking zero credit for it.

MR. MCCRITE: That's right from the containment point of view, but thinking that other people in their analyses may want to consider the pathways of water in and the pathways of radio nuclides out.

And that this is not our argument in the containment group, but as to others as being a total barrier system.

CHAIRMAN HORNBERGER: Questions from the staff? Mike.

MR. LEE: Mike Lee, ACNW staff. The Yucca Mountain rock, is that just the Calico Hills crushed tuff?

DR. HORN: Actually, I think it is Propone Springs tuff. Actually, we have isolated it from where we excavated it from Alco-5, which is in the same horizon as the repository.

NR. LEE: Okay. So it is a pretty fresh

sample then?

DR. HORN: Yes, and I just want to mention that in these studies we really have not made a distinction between organisms that are introduced as a result of construction activities, and those that are extant. So we really have not separated those out, because I don't really think it makes any difference to the project in the end.

I mean, they are going to have to deal with the whole thing. So we have tried to get it, and we have done both getting it off the surface of the walls, and inside as well.

MR. LEE: And my other question is that there is going to be a lot carbon steel possibly in the repository as a result of roof enforcement and things like that, and rock holes, and stuff.

Is there any plan on looking at the effects of microbial induced corrosion there?

DR. HORN: Well, we have done some of those studies and we did some lineal polarization and this was primarily at the time when carbon steel was the outer layer of the corrosion at the waste package.

But knowing that, there are other elements of the engineer barrier system that are close to steel and that's why we characterized the corrosion products and looked at the overall rates of corrosion.

But more recently we have frankly been focusing in more on the Alloy 22 and titanium, because it is just more of a priority.

CHAIRMAN HORNBERGER: Andy.

DR. CAMPBELL: Getting to the water issue, how much water do you need? We took a tour of Yucca Mountain yesterday and went into the cross-drift, and saw and heard discussion about mold spores. In fact, we all had to sign our life away saying that we would not hold DOE responsible.

Mold grew rapidly in that environment once it was closed up. Now, there is no liquid water there that is dripping as far as you guys and as far as DOE knows. But there is a heck of a lot of moisture there in terms of humidity and condensation.

And even without a punctured drip shield, as the waste packages cool, do you believe that there would be sufficient moisture on the surface of the waste package that these organisms could grow?

DR. HORN: Yes, I am well aware of the cross-drift issue, because when it first came up it was primarily the S&H issue, and they brought us in to do this survey of fungi. They were growing on just about everything organic down there.

And so if you look at the literature, fungi are a little more justification resistant than bacteria, but it is on the order of 95 percent Rh. Now, that doesn't include -- you know, there is some discussion that as salt brines actually build up on the package, or for that matter on the drip shield, that the deliquescence point or that point of relative humidity, where the salt actually absorb water, and produce a water film, can actually be at a lower relative humidity than that turnaround point for general microbial buildup.

So I think there are those two issues. Yes, we are saying 90 or 95 percent Rh, but that doesn't include the deliquescence point of the salt. Now, I just want to point out that if they grow in these mines, they have got to be very salt resistant organisms.

And those do exist, and I live in San Francisco, in the Bay Area, and if you have ever flown into South Bay, you will see these big salt ponds that are all red, and the reason that they are red is that there are organisms called halo bacteria that are very salt resistant, and that have these red pigments that grow in there.

So, so far we have not seen it in halo

files, or what we call halophytic or salt loving bacteria in the repository, or we have not seen them in the test kits either. So that is good news.

So how much water? Well, if it is free of relative humidity, then probably we are talking 90 to 95 percent, and all you need is a film. You don't need it to be dripping.

But then you might start at relative humidities if you have halo tolerant bacteria and you get this deliquescence on the packages or other surfaces.

MR. LEE: One other comment. In another life I actually worked on hydrothermal vent systems, and marine sediments, and in answer to Ray's question, you generally see some sort of divergence of the methane producers, versus the sulfide producers, versus the sulfate producers, and sulfide oxidizers, excuse me.

And you see a stratification in sediments, but frankly you see a lot of cross-over and you see mixtures of bacteria that in theory should not be growing together and they are, and the usual explanation was that you have micro-environments that favor either more reducing or a more oxidating environment.

And the other thing is that hydrothermal vent systems have these wonderful communities of life moving around and they are all living on essentially the bugs that oxidize sulfite, as a completely chemorodicthrophic system.

And so once you get one growing, pretty soon you colonize it with all kinds of other things, and the last thing is to remember that the reason we have oxygen in the atmosphere is because of bugs. So no matter where they are, in the earth, or even deep into the earth, one finds bacteria, and they are living off of some sort of energy source.

CHAIRMAN HORNBERGER: Don't forget there has to be an energy source, and there is a pretty darn good energy source at those vents. Any other questions nor comments from anyone?

MR. SHETTEL: Don Shettel for the State of Nevada. Are you planning to look at any other water compositions besides J-13?

DR. HORN: Well, the problem is that you look at more materials and more water -- well, we are looking at high reactions and other pHs in the context of what we saw in corrosion tanks.

MR. SHETTEL: Well, does that mean like 10 times --

DR. HORN: Actually, the more concentrated, it is a thousand times. We are attempting to expand the matrix somewhat, but it is just difficult because a lot of these are long term tests, and they take a lot of maintenance, and how to gauge that is difficult to accomplish.

MR. SHETTEL: Yes, but port water has higher sulfate and nitrate, which might be important.

DR. HORN: Well, already we know that the ground water has enough sulfate and nitrate. You almost can't have too much sulfate and nitrate for bacteria, because that is what we call macronutrient.

I mean, it is in all your proteins, and your DNA and all the membrane proteins. So you need a lot of phosphate and sulfate, nitrate, or nitrogen, and sulfur, as well as a carbon source. And those are the four things that you need a lot of.

So to increase it 10-fold wouldn't be a bad thing. It wouldn't prevent microbial growth. We are more concerned with nitrate concentrations being depleted by bacterial growth because it turns out that nitrate kind of combats chloride. Chloride generates corrosion, and nitrate sort of emolliates that effect. So the nitrate and chloride concentrations are important, and those ratios are important and is something that we are interested in looking at.

MR. SHETTEL: And my next question is that I know that you are going to try different temperatures, which is good, but when the coupons are submerged below the solution though, that is okay for anaerobic bacteria, but with the aerobic ones, you should be trying perhaps to drip the water on the coupon.

DR. HORN: We have thought about doing that. Actually, in the tanks, they are very vigorously mixed and so it is an area of environment, and it is not a closed system. It is generally closed, but it's not like it is sealed. And then these things are being continuously mixed.

MR. SHETTEL: And that would mimic a thin film, and you might find on the canister?

DR. HORN: Right. And when we sample the tanks, we actually swipe the surfaces, too, to see if we can expect more to be attached to surfaces.

CHAIRMAN HORNBERGER: I don't want to interrupt, but I don't want to carry on too much into deeply exactly what is measured, and what the plans are, because a lot of this can be done off the record. Is there another question?

MR. TYNAN: Mark Tynan, from DOE. I was

going to try to lead you to the final question. If you look at the species that you have identified from the rocks at Yucca Mountain, how do they differ form the ones that I find in my aquarium at home?

DR. HORN: Well, your aquarium is a little bit different environment. But in your garden, I would say they are a lot closer, although generally there is a lot more organic material in your garden.

MR. TYNAN: How about on the surface area at Yucca Mountain?

DR. HORN: You know, we haven't actually looked at that, and that is one of the things that I have been wanting to look at, particularly in like the playus (phonetic), these dried up salty lakes and so forth in that area, because that may be a good mimicking environment for these surface grinds that they are expecting may develop on the surface of the packages. But great question. I would love to do the experiment.

MR. TYNAN: From what you have looked at, your factor of two on C22, is that incorporated in the TSPA SR?

DR. HORN: Yes, it is.

MR. TYNAN: And is it included in SSPA and the FEIS calculations?

DR. HORN: Yes.

MR. TYNAN: And so you are adding some new things in the future that will be available throughout that will be available for LA that you indicated --

DR. HORN: Absolutely. And I know that a lot of this data is in the data bank, and we very shortly are going to be putting a lot into it.

MR. TYNAN: And then my last question is that I am leading up to is does your study indicate that long duration ventilation would be bad for the repository because of introduction of organisms that aren't there?

DR. HORN: Well, it is kind of a double-edged thing, because you are going to be introducing organisms, but you are also going to be drying things out. And I think probably the dryout factor overrides the introduction factor, because if you dry everything out, nothing is going to grow anyway.

So I think during the ventilation period it is a good thing in terms of corrosion, because it will eliminate water.

MR. TYNAN: Okay. Thank you.

MR. LEVENSON: I have one other question. You showed pictures of several different types of equipment, but just to get a feel for the scope of the program, how many specimens total do you think there is, including your long term programs?

DR. HORN: I think a couple of hundred.

MR. LEVENSON: A couple of hundred?

DR. HORN: Yes.

MR. LEVENSON: Some of the tanks have more than a hundred.

DR. HORN: Yes, but we go like into depth on each coupon.

MR. LEVENSON: No, I mean the total number of coupons you have in the program.

DR. HORN: You mean in the entire program?

MR. LEVENSON: Yes.

DR. HORN: Go ahead, Dan.

MR. MCCRITE: We have more than 20,000.

CHAIRMAN HORNBERGER: Thanks very much, Joanne.

DR. HORN: Thank you all for your attention. It has been a long day and I really appreciate it. Thank you.

CHAIRMAN HORNBERGER: I think because we had a break earlier, we are just going to continue on with our agenda. Our agenda now is open, and basically we are open for questions and comments on anything that has been heard today and actually not even restricted to anything that has been heard today. We are open to hear any questions or comments that people may have.

(No response.)

CHAIRMAN HORNBERGER: If not, very good, and thank you all for attending. We are adjourned.

(Whereupon, at 5:13 p.m., the meeting was adjourned, to reconvene at 8:30 a.m., on Thursday, September 26, 2002.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Nuclear Waste 137th Meeting

Docket Number: N/A

Location: Las Vegas, Nevada

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

/s/ Paul Intravia

Paul Intravia
Official Reporter
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